

"Let us bury the hatchet! Why should scientific men quarrel?" I signified my full acceptance of the offered peace, and great was my surprise soon after to find that, unmindful of the understood compact, he had exhumed his hatchet and was dealing me unexpected and wanton strokes, tempered by a certain amount of half praise which reminds me of the sort of caressing remonstrance of Majendie in the pre-anæsthetic days, to the dog which he had on his operating table—"Taisez vous, pauvre bête!"

In all seriousness, however, I must again ask, what is the meaning of the "personal antagonism," and the persistent attacks which Dr. Carpenter, for the last six years, has directed against me? In his recently published book, in the *Nineteenth Century*, and in his last letter to you, the key-note struck in the *Quarterly Review* six years ago is sustained. We have the same personalities, the same somewhat stale remark about my double nature, and the same exuberance of that most dangerous and misleading class of averments, half truths. Dr. Carpenter, indeed, condescends to admit that I have pursued "with rare ability and acuteness a delicate physical investigation in which nothing is taken for granted without proof satisfactory to others as well as to himself," and that I have "carried out a beautiful inquiry in a manner and spirit worthy of all admiration;" but, after granting so much, he dissembles his love and proceeds to "kick me down stairs." I am damned with faint praise, and put to rights in such a school-masterly style, that I could almost fancy Dr. Carpenter carries a birch rod concealed in his coat-sleeve. He admits that in an humble and subordinate sphere I have done useful work, only I must not give myself airs on that account. Dr. Carpenter reminds me of Dr. Johnson defending Sir John Hawkins, when he was accused of meanness. "I really believe him," said Johnson, "to be an honest man at the bottom; but to be sure he is penurious, and he is mean, and it must be owned he has a degree of brutality, and a tendency to savageness, that cannot easily be defended." In the same magnanimous spirit Dr. Carpenter allows that I have contributed a trifle to science, but he does not forget to add that I am the victim of cerebral duplicity, and I am again held up to illustrate the sad result of neglecting to train and discipline "the whole mind during the period of its development," &c.

I have, it appears, two allotropic personalities, which I may designate, in chemical language, Ortho-Crookes and Pseudo-Crookes. The Ortho-Crookes, according to Dr. Carpenter, has acquired "deserved distinction as a chemist." He carries out a "beautiful inquiry in a manner and spirit worthy of all admiration." He has shown "ability, skill, perseverance, and freedom from prepossession." He pursues "with rare ability and astuteness a delicate physical investigation." He evinces the "spirit of the true philosopher," and he has "deservedly" received "from the Royal Society the award of one of its chief distinctions."

But Pseudo-Crookes, whose career Dr. Carpenter has evidently watched almost from his cradle—as he professes to know the details of his early education—unfortunately took a "thoroughly unscientific course," and developed into a "specialist of specialists." He had "very limited opportunities" and "never had the privilege of associating" with scientific men, although he displayed "*malus animus*" "towards those with whom he claims to be in fraternity." He is "totally destitute of any knowledge of chemical philosophy, and utterly untrustworthy as to any inquiry" not technical. His "assertions" are "well known in the scientific world to be inconsistent with fact." He enters on inquiries "with an avowed foregone conclusion of his own." He has "lent himself to the support of wicked frauds." He has "prepossessions upon which clever cheats play." His "scientific tests" are not "worthy of trust." He is a believer in "day dreams," and the supporter of a "seething mass of folly and imposture;" whilst, to crown all, he actually thinks that the radiometer is driven "by the direct impetus of light." In short, this Pseudo-Crookes is a compound of folly and knavery such as has rarely, if ever, previously been encountered.

WILLIAM CROOKES (The Ortho-Crookes?)

London, October 29

Mr. Wallace and Reichenbach's Odyle

I AM amazed that Dr. Carpenter should think it necessary to make public, with such haste, Prof. Hoffmann's statement that Baron Reichenbach's facts and theories are not accepted by the

body of scientific men in Germany. Of course they are not. But how this affects their intrinsic accuracy I fail to see. Less than twenty years ago the scientific men of all Europe utterly disbelieved in the co-existence of man with extinct animals; yet the facts adduced by Freere, Boué, McEnery, Godwin Austen, Vivian, and Boucher de Perthes, are now admitted to have been trustworthy and deserving of the most careful examination. The whole history of scientific discovery from Galvani and Harvey to Jenner and Franklin, teaches us, that every great advance in science has been rejected by the scientific men of the period, with an amount of scepticism and bitterness directly proportioned to the novelty and importance of the new ideas suggested and the extent to which they run counter to received and cherished theories. Rejection is one thing, disproof is another; and I have in vain searched for anything like disproof, or even rational explanation, of Reichenbach's facts; his theory, or "Odyle-doctrine," I have never "attempted to rehabilitate," as Dr. Carpenter, with his usual misconception, says I have done. In my review of Dr. Carpenter's lectures (*Quarterly Journal of Science*, July, 1877, p. 396), I adduce five tests employed by Reichenbach, and also the independent and simultaneous confirmation of Dr. Charpignon in France; and the only reply I get is: "All men of science disbelieve them." With the facts of history above alluded to in my mind, and believing that human nature is very much the same in the nineteenth century as it was in the eighteenth, I can only say, "so much the worse for the men of science."

Dr. Carpenter's reference to the believers in a flat earth, as a parallel case, is unfortunate, because the two cases are really of a totally different nature. Those who maintain the earth to be flat do not deny the main facts which we rely on as proving it to be round, but they attempt to give other explanations of them. The dispute is on a question of reason and inference; and every intelligent and fairly educated man is able to decide it for himself. But in Reichenbach's case it is the *facts* that are rejected without disproof or adequate explanation. The two cases are therefore quite distinct, and Dr. Carpenter's attempted parallel, as well as his setting up of scientific disbelief as a conclusive reply to evidence, is in conformity with his whole treatment of this subject.

I trust that such of the readers of NATURE as may feel any interest in the questions at issue between Dr. Carpenter and myself will read my article above referred to, and not allow themselves to be influenced by Dr. C.'s repeated appeals to authority and to prejudice.

ALFRED R. WALLACE

I HAVE to request your insertion of a post-card I have this morning received, for two reasons; *first*, because, as it is anonymous, and as the writer of it is obviously a reader of NATURE, no other way is open to me for replying to it except that which your columns may afford; and *secondly*, because it is a very curious example of the misconceptions into which men are apt to fall who allow themselves to become "possessed" by "dominant ideas."

"If Mr. A. R. Wallace has to choose between being either 'a fool or a knave,' there is at all events no choice left for the man who deliberately and maliciously makes incorrect assertions and suppresses the truth to further his own views. I dare say you know what most people would call such a man. Yours,

"ONE WHO WAS AT PLYMOUTH"

Now, in the first case, it must be perfectly obvious to any one who is capable of reasoning logically, that nothing which I said of Mr. Wallace in your last number can be twisted into the implication that he is either "a fool or a knave." John Hampden is continually saying this of Mr. Wallace and of everybody who upholds the rotundity of the earth. And I mildly suggested whether, in putting himself in opposition to the whole aggregate of scientific opinion on the value of Reichenbach's Odylism—not because he had himself repeated them, but because he believes in Reichenbach—Mr. Wallace is not assuming an attitude in some degree similar, that is, setting himself up as the one wise and honest man who duly appreciates Reichenbach, and therefore implying that everybody else is either stupidly or wilfully blind to the evidence he presented. If anyone thinks it worth while to read Mr. Wallace's review of my lectures on "Mesmerism, Spiritualism," &c., in the last number of the *Quarterly Journal of Science*, he will be able to judge whether I have or have not wronged Mr. Wallace in this matter.

The writer's appreciation of my own character, which has fre-

quently been expressed to me before in the same manner and in the like terse and elegant language, is now enforced by what he deems to be Prof. Carey Foster's judicial opinion, delivered at the Plymouth meeting; and I find myself, therefore, fully justified in my opinion that by his introduction of the word "intentionally" Prof. Carey Foster made his judgment legitimately bear a meaning, which, as he has stated, he would consider insulting to my character. And I cannot but believe that Prof. G. Carey Foster will regret having thus given a new handle to a man who obviously *wishes* to insult me on account of my antagonism to spiritualism. As the writer of the post-card continues to use Prof. G. C. Foster's authority, *after* that gentleman's explicit disavowal of the offensive meaning here attached to it, and as I may, of course, expect that he will continue to avail himself of that authority, I should like him to know through your columns that it is scarcely worth while for him to trouble himself to repeat these attacks, since they have long since ceased to do anything else than amuse me, and will only furnish me with materials for amusing other people.

It seems much to be regretted that neither spiritualism nor attendance at the meetings of the British Association, nor even the reading of NATURE seems able to teach this person to behave like a gentleman.

WILLIAM B. CARPENTER

October 29

Potential Energy

YOUR correspondent "X." has described some of his troubles respecting potential energy. Many a learner could describe similar troubles respecting force and energy in general. They who earnestly contend for definiteness and accuracy do not always teach with definiteness and accuracy. For example: in his "Treatise on Heat," p. 137, Dr. Tyndall tells me that by raising a weight from the floor I have conferred upon the weight potential energy. Presently he tells me that this energy is derived (not from me, but) from the pull of gravity. He next tells me that we might call the energy with which the weight descends, moving force, *i.e.* he teaches me to confuse force and energy; and after all this he bids me remember that "exactness is here essential. We must not now tolerate vagueness in our conceptions."

Take another example. In his lecture on "Force" (NATURE, vol. xiv. p. 462), Prof. Tait teaches that force is a mere name, and that it has no objective existence; he also teaches that the product of this non-existence by its displacement has an objective existence. Few learners would say that is a very lucid statement. Again, in the same lecture he says "there is no such thing as centrifugal force, and accelerating force is not a physical idea at all;" but in his "Nat. Phil." he speaks of both these forces, and describes their effects (Nos. 185, 187, 598, 248).

When teachers deservedly eminent make statements like the foregoing, so likely to mystify and confuse a novice, it is no wonder that there is a good deal of smattering in popular science.

Prof. Tait says "the so-called accelerating force is really no force at all, but another name for the kinematical quantity acceleration." I venture to entirely disagree with this statement, and

for the following reason:— $\frac{dv}{dt}$ is a number, and may be that

number of units of force, or that number of units of acceleration. When it is called accelerating force it is the representative of

$m \frac{dv}{dt}$, when $m = 1$, and m does not appear in the expression;

and it means $\frac{dv}{dt}$ units of force. When it is called acceleration

it means $\frac{dv}{dt}$ units of acceleration. Accelerating force is just as

real as moving force, for it is, in fact, the m th part of the moving force. In like manner v may mean either v units of velocity, or v units of momentum; in the latter case it is the representative of mv , when $m = 1$, and means the momentum of a unit of mass which has v units of velocity. In like manner m may mean either m units of mass, or m units of momentum, or m units of kinetic energy; in the two latter cases it is the representative of mv or of mv^2 when $v = 1$, and means the momentum, or the *vis viva* of m units of mass moving with unit of velocity.

A few simple definitions would remove the difficulties respecting force. Thus: If a mass of m units of mass is at any

instant receiving an acceleration of a units of acceleration in any given direction, the force which is acting on it at the given instant in the given direction is ma units of force. The force acting on the mass in the direction of its motion is called the moving force. The force in the normal to the direction of its motion and towards the centre of curvature is called the centripetal force. An equal and opposite force is called the centrifugal force. The m th part of the moving force is called the accelerating force, which is the moving force acting on a unit of mass.

In the case of a planet's orbit it is too common to give the name centrifugal force to two forces which generally differ both in magnitude and in direction, one of them being in the direction of the normal, the other in the direction of the radius-vector. This is the last instance which I shall give of sins against definiteness and accuracy.

E. G.

Bardsea

Hartlaub's "Birds of Madagascar"

THE excellent review, exhibiting traces of a master's hand, of the above-named useful work, which appeared in NATURE (vol. xvi. p. 498) prompts me to offer some remarks on the ornithology of Madagascar and its neighbouring islands, and to take exception on two points therein laid down.

The first of these is propounded by your reviewer and seems to me absolutely contrary to fact. He says:—"Compared with Madagascar itself the appendant island groups are poor in species, although in every case there are many interesting forms among their winged inhabitants. The Comoro Islands muster only some forty-four species of birds, Mauritius about sixty, of which fifteen or sixteen have been introduced by man's agency, and Bourbon about the same number, while Rodriguez appears to have only about twenty-five species now existing in it, of which four or five are certainly recent introductions."

Now twenty years ago my friend, Mr. Sclater, in that remarkable paper of his on the geographical distribution of birds (*Journ. Linn. Soc. Zoology*, ii. p. 130), which so happily laid the true foundation for our present researches into the subject, showed that the proper mode of comparing the wealth or poverty of one fauna with another was to state the proportion which the number of species composing it bears to the area over which they range. The same view was adopted very shortly after by Mr. Wallace, who took occasion (*Ibis*, 1859, p. 449) to question certain of Mr. Sclater's results, and its correctness seems to have been since generally admitted. Yet, applying this test to Madagascar and its neighbouring islands, we find a state of things to exist very different from that which your reviewer has alleged. The area of Madagascar is said¹ to be 10,751 German square miles, that of the Comoros collectively 38'57, of Mauritius 34'76, of Bourbon 42'05, and of Rodriguez 5. It will be sufficient for my purpose to compare the first and last of these. Your reviewer is willing to allow twenty indigenous species to Rodriguez; then—

Area of Rodriguez.	:	Area of Madagascar.	:	Species in Rodriguez.	:	Species in Madagascar.
5	:	10,751	:	20	:	x
$x = \frac{10,751 \times 20}{5} = 43,004.$						

But instead of an avifauna of 43,004 species, or about four times the number known to exist throughout the whole world, Dr. Hartlaub gives it 218, and your reviewer generously adds two more, making 220! Suppose (an extravagant supposition) that future explorations enable us to double the last number, it is Madagascar that will still be out of all proportion "poor in species" compared with "the appendant island groups," and not these with Madagascar.

The next point to which I must demur is that "the individuality of the fauna of Madagascar is so unique that even that of New Zealand can hardly be compared with it." I will leave to fitter hands than mine to show that this is not the case generally, and shall only remark here that it is not so with birds. Of the sub-class *Ratitæ* there have been until lately five strongly-marked groups, each of which is equivalent to an "order" among the *Carinatae*. Now two of these groups were peculiar to New Zealand, and one (*Apterygidae*) is so now, while the other (containing the families *Dinornithidae* and *Palapterygidae*) is but recently extinct. Willingly granting that *Aepyornis*, when we

¹ Behm und Wagner, "Areal und Bevölkerung der Erde" (Petermann's Geogr. Mittheilungen, Ergänzungsheft, November 20, 1876).